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THE AMERICAN
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VOL. V. ANN ARBOR, MAY, 1888. NO. 1.

ORIGINAL ARTICLES.

THE METEOROLOGICAL LABORS OF DOVE, REDFIELD AND ESPY.

BY MAXIME BÔCHER.

Meteorology is a young science. Indeed it is only within the last thirty years that it has become in any sense one of the exact sciences, that is, one susceptible of mathematical treatment. It is Mr. Ferrel more than any one else, who has actually brought about the change which has occured in this direction; but it is really due fully as much to the careful observations and sagacious theorizing which began sixty years ago. It is the theories and observations which led to this change in the character of meteorological science that I shall attempt to describe.

The greater part of the work of the three meteorologists of whom I shall speak is included between the years 1830 and 1860; a formative period during which the scientific world was divided between the most opposite theories, a period characterized, in this science, by the use of no mathematics higher than arithmetic. The subjects which occupied the minds of meteorologists at this time, to the exclusion of everything else, was the theory of storms, both of wind and of rain; a region in which they found the paths as yet almost untrodden. The preceding meteorologists had done some valuable work, of which the most important was perhaps Hadley's partially correct explanation of

the trade winds of the torrid zone; but they had not even a correct understanding of the motions of the atmosphere in a storm, much less could they explain them. In regard to rainstorms, however, there was one plausible theory, that of Hutton, who, towards the end of the eighteenth century, had shown that when two masses of air at different temperatures are mixed together, a portion of their moisture may be precipitated; so that the meeting of a warm and a cold wind would be the cause of a rainstorm. With the exception of this, we may fairly say that nothing was known about storms except a few facts of a popular character, such as that a northeast gale on our coast "backs up" against the wind.

In 1821 William C. Redfield had already passed the age of thirty, and nothing seemed more unlikely than that he should one day be regarded as one of the fathers of the modern theory of storms. A mechanic by trade, with only a common school education, he had as yet given no attention to any branch of science. But in the fall of that year he had occasion to go from his home in Connecticut to the western part of Massachusetts, over the very region which a few months before had been swept by a violent September gale. The wind had left its record in the shape of fallen trees, and as Redfield advanced and found the trees lying in the opposite direction from those near his home, it flashed upon him that the gale was a whirlwind on a large scale which advanced over the country from southwest to northeast, while at the same time it whirled around its axis in a direction opposite to that in which the hands of a watch move.

This was Redfield's great discovery; and, as was the case with Darwin when he discovered the principle of natural selection, it came to him in a moment, almost like an intuition, only to be confirmed by years of patient labor and arduous collection of facts. But unlike Darwin, Redfield did not have his time at his own disposal, and it was not until the year 1831 that he was enabled, with the encouragement of Prof. Olmstead, to publish his first meteorological paper in the *American Journal of Science.* This article is in great part devoted to the consideration of the storm of ten years before, and contains a con-

siderable number of independent observations extending over all our Atlantic coast. He thus puts his whirlwind theory on a firm foundation, and he also makes his second great discovery, namely, that these storms move, roughly speaking, in a parabolic path; starting to the eastward of the islands of the Caribbean sea, and moving northwest and gradually north until they reach 30° of north latitude, when their path becomes more and more easterly. These two important principles are here established independent of any theory. In fact the theory that Redfield himself gives in this paper—that these whirlwinds are eddies of the atmosphere formed by the trade wind impinging on the land—was entirely erroneous, and was soon cast aside by its author. In a paper on the hurricanes of the West Indies, published two years later, Redfield recapitulates the facts that he had established in his previous paper, and also gives some important advice to sailing masters as to how they may avoid these great storms.

However decisive the array of facts brought forth in these early papers may appear to us, it seems to have been very far from carrying conviction to the minds of several contemporary meteorologists. Of these the most notable was Espy, who from this time forth appears as the persistent opponent of Redfield, both as to facts and theory. He, like Redfield, saw that the wind blows from different quarters in different parts of the same storm, but he accounts for this by supposing that the wind always blows inward from the edge of the storm to a central point or line. Moreover, according to Espy, the air rises at the centre of the storm to "the surface of the atmosphere" and there spreads out. The theory which he has constructed to explain these supposed facts is, strange as it may seem, in good part the true one; while Redfield, whose observations were accurate, held until the end of his life to theories most of which were false.

As early as 1828 Espy saw the importance of observations on the dew point and the amount of vapor contained in the air, but it was not until 1836 that he first made known his theory of storms in some papers published in the journal of

the Franklin Institute. As there stated, his theory is as follows:

"When a portion of transparent vapor, in the air, is condensed into cloud or water, the latent caloric given out expands the air containing it six times as much as it contracts by the condensation of vapor into water;" so that "the moment a portion of transparent vapor in the air begins to condense into cloud, the air in which it is contained begins to expand, and, consequently, if an equilibrium existed before, it is now destroyed, and the cloud will continue to ascend as long as its temperature is greater than that of the surrounding air." This difference in temperature between the rising air and the adjacent air through which it rises will increase until all the vapor is condensed into cloud. From this point this difference remains unchanged, and the column of air will rise to the surface of the atmosphere, where it will spread out on all sides.

In this theory there are two fallacies. First, the latent heat given out when vapor condenses can never cause an actual rise in temperature, but will merely check the fall of temperature which was producing the condensation; so that we must have an original difference of temperature near the surface of the earth in order to start the ascending column of air. Espy himself soon saw this, and altered his theory slightly in his subsequent writings. The second fallacy consists in neglecting the fact that rising and consequently expanding air cools at the rate of 1.6° for every 300 feet of ascent, while the temperature of the air through which it passes falls only 1° for every 300 feet of ascent; so that, unless prevented by the condensation of vapor, the temperature of the warm rising air will approach the temperature of the colder surrounding air at the rate of 0.6° for every 300 feet of ascent, and there will be a point above which the column of air will not rise. The rate of cooling of expanding air was not accurately known, hence Espy's error.

But to return to the theory. As this air rises, other air will rush in from all sides to take its place, and this will in turn rise, thus forming what Espy calls a vortex, near whose centre there will usually be rain, and towards whose centre the wind will

blow; thus according perfectly with the facts of the case as Espy believed. Moreover, it explained not only a wind-storm with a rainy centre, but also a hail-storm, when the raindrops are carried up by the ascending current into the region of perpetual congelation and then thrown out at one side; water-spouts and land-spouts, which are merely small vortices of unusual violence, which reach down with their accompanying cloud, as far as the earth; and lastly, but most important of all, the diminution of barometrical pressure at the centre of storms, for here we have a column of relatively warm and consequently light air.

It was in the third of these early papers, published in the journal of the Franklin Institute for August, 1836, that Espy opens the discussion with Redfield which was to last for twenty years. Redfield had already published four or five papers, in various journals, in which he stated more clearly the position he held in his first paper, and also brought forward a great many observations on a number of other storms, all of which confirmed his whirlwind theory. Espy, however, confines his attention to Redfield's first paper, and by taking advantage of an inaccurate statement and supplementing this assumption by bad logic, attempts to show that Redfield's storm was centripetal and was not a whirlwind. He then goes on to prove mathematically that the centrifugal force of a whirlwind would be sufficient to throw out the air from the centre of the storm and thus cause a downward suction at the centre which would prevent any rain there. In making this objection, Espy forgets that Redfield has nowhere said that there is no centripetal force to balance the centrifugal, but merely that there is no centripetal motion. In fact, the fundamental distinction between the two men is clearly shown here. Espy looks at the *forces*, Redfield at the *motions*. Espy builds up theories on well-known physical principles and works over his facts until they fit the theories; Redfield looks at the facts first and then attempts to construct a theory for them. In a word Espy's method is deductive, Redfield's inductive.

It was not long after this attack that Redfield's answer came

through the columns of the same journal. After replying to Espy's misrepresentations of the facts, he turns to the theory and objects that if the air rushes from all directions towards a point, the barometer should rise instead of falling, as it actually does—forgetting that according to Espy's theory the air flows away at the top as fast as it flows in at the bottom. I merely mention this as showing the looseness of Redfield's physical ideas whenever he attempts to consider theories; but another, and far more important objection of Redfield's, and one of which he himself did not see the consequences, is the following.

If a circular tub is filled with water which is then allowed to escape through a hole in the bottom, the water will move, not directly towards the hole from all sides, but in a spiral eddy. Now, Espy's storm theory corresponds exactly to this, for in it we have a mass of air which drains vertically upward through a central point, instead of a mass of water draining downward through a central point; so that the resulting phenomena should be the same, and we ought to have a spiral motion inward and upward, instead of Espy's radial motion. That Redfield did not see the full bearing of this fact is evident, for, if he had, the discussion which was to last the rest of his life would have stopped here, and he would have adopted Espy's theories as explaining his own facts, for he had already noticed that the storms he had investigated did not blow in exact circles about a central point, but that the wind constantly approached that point, thus forming an inward spiral.

In some of his later papers Espy considers this objection, and says that he has performed the experiment with the tub of water, and found that if care was taken to have the water still before opening the hole, no perceptible rotation was produced. It is, of course, difficult to see how nearly this condition is satisfied before a storm, and hence how much rotation a storm should have. Espy considered it probable that a few of the small storms, such as tornadoes, do revolve, but he retained, till the last, his belief in the centripetal motion of the wind in most storms. It was afterward shown by Tracy and Ferrel that the rotation of the earth supplies the required disturbance to pre-

vent simple centripetal motion, and, moreover, to determine the direction of rotation,—from right to left in the northern, and from left to right in the southern hemisphere. Redfield attributed the whirlwind nature of storms, as well as most other motions of the atmosphere, to the rotation of the earth, but he seems to have thought that the motion itself and not merely its direction was due to this rotation. But although his explanation of the cause of the rotation of the air in a storm was wrong, he still clearly perceived its effects, one of which is the increasing violence of the wind as one approaches the centre of the storm. This fact is due, as he pointed out, to the law of conservation of areas, or as he calls it, the law of equal areas in equal times.

It is not worth while to follow out the discussion between Redfield and Espy in detail, for we should merely see the same arguments and methods employed again and again. Each developed his theory, but without regard to the criticism of his opponent, and the discussion appears mainly to have served the purpose of stimulating each to strengthen every point of his theory, not to sift the true from the false.

As early as 1834 Redfield expressed the opinion that when the typhoons of the China seas should be investigated they would be found to be similar to the West India hurricanes, both in their internal structure and in the path they pursue. He also said at this time that he considered it probable that the storms of the southern hemisphere would be found to rotate in the opposite direction from those of the northern hemisphere; and also that their path would be found to be the reverse of that of the West India hurricanes, that is, starting in the tropics and moving towards the southwest, then south, southeast, and east. Although this can hardly be considered as more than a fortunate guess, resting as it must have done on inaccurate theories, still it was found to be true a few years later when the facts of the case were investigated. It was not many years after he had published his first paper that Redfield began to receive assistance from all parts of the world. Ried in the West Indies, Piddington in the Bay of Bengal, and Thorn in southern Indian

ocean, are among the most celebrated of those who, inspired by his writings, have contributed a mass of precious information to the theory of storms.

But if Redfield had the coöperation of scientific men the world over, Espy had the advantage of government aid, for in the year 1843 he was appointed to a position in the war department for the purpose of collecting facts about the great storms that sweep the Atlantic coast of this country. In this position he published four "Meteorological Reports," which, together with his "Theory of Storms" published in 1842, complete the list of of his writings. In these five volumes he has given his theory but little development, the chief additions being in the investigation of a great number of individual storms which he tried to show were centripetal; if not towards a point, at least toward a central line. It will thus be seen that he held this erroneous opinion to the last; whereas Redfield had modified his original belief in the absolutely circular motion of the wind, as set forth in his first paper, and gave an ever increasing weight to the spiral nature of these great storms, tending inward and upward; and in one of his papers published not long before his death, which occurred in 1857, he mentions the fact that these spirals approximate more closely to circles than to the radial lines of Espy.

We must now pass on to the third of the meteorologists whose names appear in the title of this article. Dove, a German, was born in 1803, but, though nearly twenty years younger than Redfield or Espy, he began to write before either of them, and continued until his death in 1879. He received a university education, and the first paper he published was the thesis, for which he received his doctor's degree, entitled "De Barometricis mutationibus." In this paper—which appeared before Redfield's first publication, but five years after his observations and discoveries in relation to the storm of the Connecticut valley—Dove announced his theory of storms. This was essentially the same as Redfield's, that is, that storms are great progressive whirlwinds turning from right to left, and was founded on observations of European storms. We thus have, what so

frequently occurs in science, the almost simultaneous but quite independent discovery of a great principle by two different men; but fortunately it did not this time give rise to the futile and often unseemly discussion as to who should have the honor for the discovery which too often takes place on such an occasion.

Dove's work is of a much more complex character than that of either Redfield or Espy, for he considers not only storms, but also the general subject of winds. It will be best to consider the latter subject first, for on it he builds up his theory of storms.

His theory of the trade winds he takes unaltered from Hadley. According to this the heated air at the equator rises, thus causing a horizontal inflow from the torrid zone on each side. But this inflow, which we should expect to find as a north wind in the northern hemisphere and a south wind in the southern, is changed by the rotation of the earth into a much more easterly wind, thus producing the well known northeast and southeast trades. The air which rises at the equator flows outward at a considerable elevation, and owing to the rotation of the earth forms the southwest and northwest anti-trades of the upper atmosphere in the torrid zone. These winds were supposed by Dove, and many other meteorologists of this period, to descend near the tropics to the surface of the earth, and to blow thence towards the poles, becoming as they went more and more westerly. But in order to account for the return of this air from the poles Dove supposes that there are, in the temperate and arctic regions, cold polar currents appearing as northerly or easterly winds in the northern hemisphere, and as southerly or easterly winds in the southern. This idea of polar and equatorial currents placed side by side, instead of one over the other, as is the case in the torrid zone, was the fundamental point in Dove's theory of the atmospheric circulation. He held, moreover, that these two currents do not remain fixed in the same geographical localities, but are constantly in motion, one of them after blowing a while over a definite locality, being displaced and giving way to the other, and thus producing the irregular changes of temperature which he has discussed in his book, en-

titled "Untersuchungen über die nicht periodischen Aenderungen der Temperaturvertheilung auf der Oberfläche der Erde."

He says that the equatorial current predominates for two reasons: first, because the meridians converge towards the poles so that the air from the equator moves in ever narrowing channels, and hence, encroaches on the polar currents, which move in ever widening channels; and secondly, because the air from the equator is warmer, and therefore occupies more space, if we consider equal masses, than the cold air from the poles. This predominance of the equatorial current explains the prevalent southwest winds of the north temperate zone.

Starting with these theories, Dove shows that when one current is displaced by the other, the wind will pass from west to east through north, or from east to west through south, and this is what he calls the law of gyration. In the southern hemisphere the direction of rotation is reversed. That the wind changes in this direction is a popular belief of sailors the world over, as is shown by the rhyme:

> "When the wind veers against the sun,
> Trust it not, for back it will run."

This fact, on which Dove laid great stress, is recognized by modern meteorologists as true only in certain parts of the world, such as the greater part of Europe and the United States, and is there explained by the habitual passage of storm centers to the northward. But it is not considered to be universally true, as Dove thought it was.

The only other subject related to the general circulation of the atmosphere which Dove discusses, is that of the Monsoons, of which those of India may be taken as the type, which blow from the southwest, replacing during the summer months the northeast trade wind. This Dove explains thus: During the summer the sun is north of the equator, so that it is here that we must look for the hottest belt of the earth, which we will call, for convenience, the heat equator. Now it is towards this heat equator that the winds blow, forming, when it coincides with the true equator, the ordinary trade winds. But in summer the southeast trade wind, blowing towards the heat equator

from the south, is obliged to cross the true equator, and to blow for a considerable distance in the northern hemisphere. Here the rotation of the earth acts upon it in just the opposite way from what it did in the southern hemisphere, and changes it from a southeast into a southwest wind—the southwest monsoon. This is one of the few points in Dove's theory which have been substantiated by subsequent investigation; whereas, the theory of polar and equatorial currents in the temperate zones, on which he laid more stress than on anything else, has been shown to be without any foundation. It is now generally admitted that all non-periodic changes of weather are due to storms of the same general character as those described by Redfield.

It should not, however, be inferred from this that Dove discarded entirely the whirlwind character of storms. As has already been said, the discovery of their rotary nature is due to him as much as to Redfield, and at first he laid as much stress on it as Redfield did, believing that all storms were of this character. In fact he is the first to have discovered by actual investigation that the storms of the southern hemisphere rotate in the opposite direction from those of the northern. In his later papers, however, he lays much less stress on this kind of storm, believing it to be only one of many varieties. He assigns as the cause of these storms, which he calls cyclones, and which he saw originated in the torrid zone, the local descent of the upper southerly current into the lower region of the trade winds, where there results a conflict of almost opposite winds which, he supposed, produced a rotation or whirlwind. It will thus be seen that he considered these storms to be a kind of eddy in the atmosphere, thus holding much the same opinion as Redfield on their essential nature. Moreover, he adopted Redfield's discovery of the parabolic track pursued by these storms. The discovery of this track was due entirely to Redfield, who, it should be said, was placed much more favorably in America for observing it than Dove was in Europe.

Dove soon saw that there are storms in the temperate zones which do not come from the tropics, and he explained them by

supposing that they are caused by the conflict of a polar and an equatorial current moving in opposite directions. He seems to think that these may or may not be whirlwinds, and he distinctly states that they do not advance over a definite path like tropical cyclones, but remain nearly stationary, alternately advancing and receding according as the polar or equatorial current predominates. At present it is well known that all the storms of the temperate zones are cyclones, but that only a few of the most violent and symmetrical have a tropical origin. The others, however, have almost as definite a path as these, their motion being in general from west-southwest to east-northeast. Some of them have been traced nearly around the globe from the Rocky Mountains across the Atlantic and into Siberia. As regards their original cause we are still in ignorance, but we can safely say that they are not due to equatorial and polar currents, since no such currents exist, at least in the shape supposed by Dove. They are *maintained*, however, by their warm, moist center, as are also the tropical storms, and thus give another illustration of Espy's theory. It is interesting here to notice that in one of his early papers Redfield suggested that these less violent storms were probably of the same whirlwind character as the others which he had studied; but, owing to their greater irregularity, and to the fact that they came from a region at that time thinly settled, he was unable to trace them to their source, or even to investigate them carefully.

It will be seen by this sketch of the meteorological opinions of Dove that only a small part of his theories have survived to the present day; but it should not be inferred from this that they were useless, for they were an attempt to form a consistent system of atmospheric phenomena, standing firmly on observed facts on the one hand and on physical laws on the other. For this task he was well fitted, as he held during the greater part of his life the position of Professor of Physics at the University of Berlin. Strange as it may appear in view of this fact, he was never willing to acknowledge that there was any truth in Espy's theories as to the action of heat, whether latent or actual, in producing storms. But in this respect he was not as bad as Red-

field, who even went so far as to deny that the trade winds are due to heat; and seems to have thought that all the motions of the atmosphere could be explained by means of the rotation of the earth. He frequently says that he regards heat as hardly more a cause of storms than electricity, against the use of which in explaining such phenomena both he and Dove have often argued. But it should be said that Redfield laid much less stress on his theories than Dove did on his, which were less absurd if hardly more correct; whereas Espy, whose theories stand in many respects unchanged to the present day, considered them of even more value than the facts. Dove stands in many ways between the other two meteorologists of whom I have spoken. They go to the opposite extremes in all things, each doing admirably what he does do; while Dove is more moderate, less absolute; but as though to compensate for this, he is also less profound. In a word, he did not take such a simple view of things as they did; his merit lies in breadth of view, not depth of insight, which is what redeems the narrowness of view of both Redfield and Espy.

THE AUSTRIAN METEOROLOGICAL STATION ON THE SONNBLICK (10,170 FEET).

By A. Lawrence Rotch,

Member of the German Meteorological Society, and Fellow of the Royal (London) Meteorological Society.

The meteorological station on the Sonnblick is the highest in Europe, and the utmost value attaches to the observations from their discussion by Professor Dr. Hann. A brief description of this notable station was given by the writer in the Journal, No. 8, Vol. III. The following account is based upon his visit in September, 1887, with Major von Obermayer and Herr Rojacher, supplemented by articles which have appeared in the *Meteorologische Zeitschrift.*

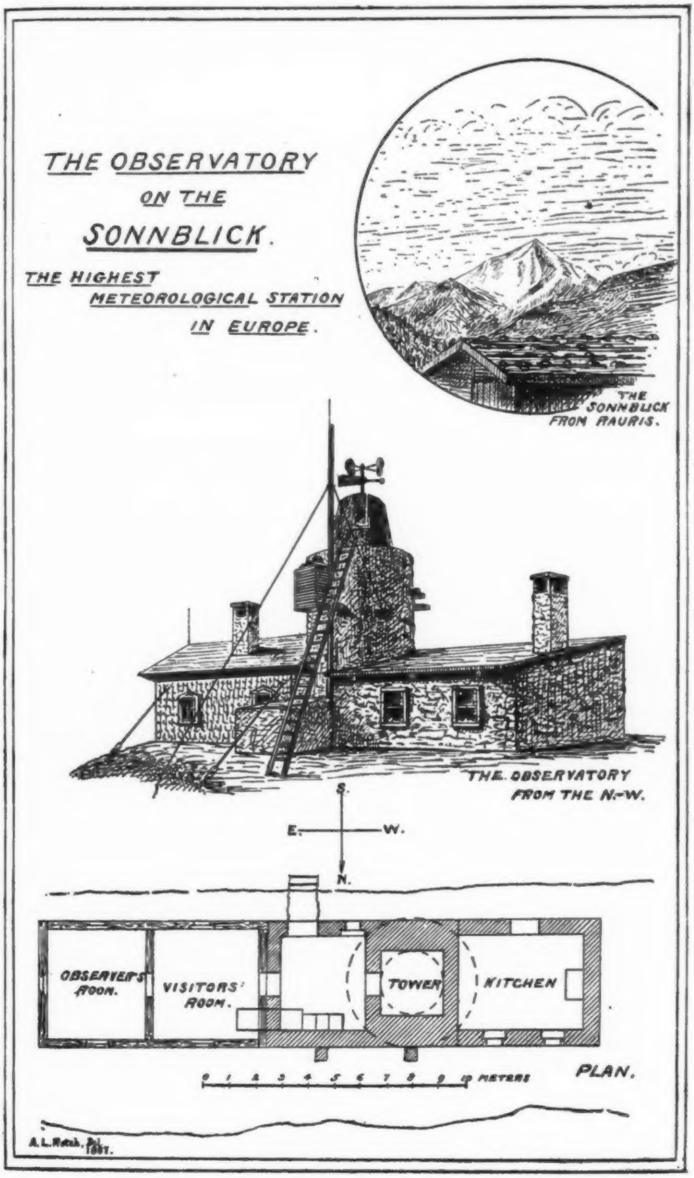
The Mountain.—The Sonnblick, in the province of Salzburg, lat. 47° 3′ N., long. 12° 57′ E., belongs to the Hohen Tauern group of the Austrian Alps. It closes the Rauris valley on the

south, rising 1,500 m. above it and about 3,100 m. above the sea. Its summit is a sharp trapezoid of bare rock flanked on the north and west by precipices. From the "cirques" thus formed spring the north glacier, and that of the Goldberg on the east extending down to the Knappenhaus. It is over the latter glacier that the ascent of the Sonnblick is generally made. Though standing as a pyramid comparatively isolated, in the vicinity are a number of other peaks having nearly the same or a greater altitude.

History of the Station.—The plan proposed in 1884 by Herr Rojacher, the proprietor of the gold mines at Kolm-Saigurn, and carried out with the help of the Central Meteorological Institute, was the establishment of a station of the third order at the Knappenhaus (2,340 m.) with which a cable railroad and a telephone make communication easy. The temperatures here were found to be influenced by insolation, and Herr Rojacher then decided to have a station upon the summit of the Sonnblick (3,100 m.), preliminary winter trips having shown this mountain to be better adapted for the purpose than the neighboring Herzog Ernst (2,933 m.). As the Central Institute could appropriate no money for a building, Director Hann laid the project before the Austrian Meteorological Society, by which it was enthusiastically received. The German-Austrian Alpine Club also signified its willingness to co-operate in the work.

The winter of 1885–86 was utilized to fit together the wooden house below. Its cost was borne by the Alpine Club, while the Meteorological Society undertook to build the stone anemometer tower, to lay the telephone line from the summit to the Knappenhaus, and from Kolm-Saigurn to Rauris—in all a length of 25 km.—also to procure the necessary registering instruments for the summit and base stations. For this purpose subscriptions were opened, and up to January 1, 1887, 4,260 florins had been received. To this must be added 1,700 florins voted by the Meteorological Society, making a total of 5,960 florins.

The early summer of 1886 was very unfavorable for building, and the work was prosecuted under great difficulties. The weather was wet and the mean temperature below freezing. All


THE OBSERVATORY
ON THE
SONNBLICK.
THE HIGHEST
METEOROLOGICAL STATION
IN EUROPE.
THE
SONNBLICK
FROM RAURIS.
THE OBSERVATORY
FROM THE N.-W.
S.
E.
W.
N.
OBSERVER'S ROOM.
VISITORS' ROOM.
TOWER
KITCHEN
0 1 2 3 4 5 6 7 8 9 10 METERS
PLAN.

the materials had to be dragged up 800 m. over a glacier furrowed with crevasses, each trip requiring three or four hours' time. The reflection from the snow blinded the laborers, and before the installation of the lightning conductor they were prostrated by lightning strokes. If it had not been for the energy and skill of Herr Rojacher the building would not have been completed that summer. It was formally opened September 2, 1886, in the presence of many invited guests.

The Building.—The actual summit of the mountain has been enlarged by retaining walls so that it now presents a level surface about 10 m. by 30 m. in area, on which stands the Observatory. The wooden house first built contains the room of the observer (also intended for visiting scientists) and another room for tourists, etc., which was formerly used as the kitchen. Each of these rooms is about 4 m. square, and in the latter is a large brick stove. The walls of this house are of beams with the joints caulked with moss, shingled outside and sheathed inside. The windows are double, and those in the observer's room face north, east and south. In the loft formed by the gable-roof are eight beds for tourists. This house is anchored to the rock by iron rods with turn-buckles. Adjoining it on the west is the two-story stone tower, resting on a massive foundation, through which is the principal entrance to the Observatory. The circular tower is about 4 m. in diameter outside, and is surmounted by a wooden truncated pyramid, sheathed with copper, which carries the cups of the anemometer and the wind vane, at a height of about 10 m. above the ground. The foundations of the tower have been extended west and covered with a pitch-roof forming the kitchen. It is proposed to further extend this in order to furnish additional accommodation for tourists.

A pole rising above the tower carries a lightning-rod. Another rod on the east gable has two branches, all being finally connected with the ground conductor, which extends down 2,200 m. over the ice and rocks to a small lake. This line also serves as the ground for the telephone, whose wire is carried up from the Knappenhaus upon poles fixed in the glacial moraines.

Much difficulty is experienced in winter from the breaking of this wire.

The Instruments and Observations.—In the observer's room, at a height of 3,095 m. above sea-level, are two Fortin barometers made by Kappeller, an aneroid barometer, a Redier and a Richard barograph. Built from the north window of the second story of the tower is a louvre thermometer screen with a ventilated roof and closed bottom, and separated by double glass doors from the interior of the tower. It contains wet and dry bulb thermometers, vertical maximum and minimum thermometers by Kappeller, and a maximum by Haak, a Koppe hair hygrometer, and a Richard thermograph and hygrograph. In the tower the anemograph, constructed by Schneider, of Vienna, registers the revolutions of the Robinson cups and the movements of the vane, which are exposed on the cupola. The registration is effected mechanically, the motion of the cups turning a shaft which by gears punctures a paper strip for each 2½ km. of wind movement. Clock-work unrolls the paper strip and each hour marks the time-scale on it. The direction of the wind is continuously recorded on the paper by eight pencils operated by cams on the axis of the vane. A Campbell-Stokes sunshine recorder is exposed outside the south window of the tower.

Eye observations are made at 7 A. M., 2 and 9 P. M., and recorded upon the usual schedule of the Central Institute. The reduction of the eye observations and of the sheets of the self-recording instruments is done in Vienna. The maximum thermometers are read and set at 2 P. M., the minimum at 9 P. M. The wind's force is recorded on a scale of 5, based on the number of revolutions per minute of the anemometer shaft. The direction of the wind is recorded to 32 points, and the cloudiness on a four-part scale.

The precipitation is not measured on account of the impossibility of getting accurate results, but its occurrence and species is noted. The morning and the evening observations are telephoned each morning in the international cipher, to Kolm, whence they are sent to Vienna in time to appear in the after-

noon Telegraphic Weather Report. At the time of making the observations a telephone signal is given to Kolm that simultaneous observations may be made there. The instruments at Kolm, which is 1,500 m. below the Sonnblick and 2,500 m. distant horizontally, are a Fortin barometer, a Richard barograph and thermograph, which latter instrument, together with the psychrometer, is exposed in a metallic screen outside a second-story window on the north side of the Kolmhaus.

Cost and Maintenance of the Station.—The first cost of the Observatory was as follows: The wooden house built by the Alpine Club, 1,800 fl., to which the Austrian Tourist Club added 600 fl. for furnishing; the masonry tower, 1,076 fl.; the telephone line, lightning conductor, etc., about 2,244 fl.; the meteorological instruments, 600 fl., and their installation, 300 fl.; all of these expenses being borne by the Meteorological Society.

The annual cost of maintenance is about 1,200 fl., which it is hoped the State will assume. The observer is paid 40 fl. per month and has free board and lodging. In summer there are at the station an innkeeper and a cook, but in the winter the observer lives alone. The first observer was a miner named Neumayer, the present one Gatzner by name. The observations at the Kolmhaus are conducted by Herr Rojacher or his wife. The former also superintends the work of the summit station under the advice of the Central Institute.

Results of the Observations.—The observations to August, 1887, have been discussed by Prof. Dr. Hann in the *Meteorologische Zeitschrift*, and for the first complete year in a paper presented to the Vienna Academy of Sciences, January 5, 1888. The tri-daily observations of the first seven months have been printed *in extenso* and will probably appear in the *Jahrbuch der k. k. Central Anstalt für Meteorologie.* This station, therefore, though the newest of the mountain stations, has already proved more useful than any other.

The mean annual temperature is computed to be about—6.7° C. In 1887, February was the coldest month, its mean temperature of —15.7° corresponding to the normal January temperature of Orenburg. The absolute minimum was —32° and the

absolute maximum 8° in July. The mean summer temperature was 0.3°, which is the lowest known for any place in the Northern Hemisphere.

From the first months' observations here and at neighboring mountain and valley stations, Hann found that the changes of temperature with elevation between 1,800 m. and 3,100 m. is almost entirely independent of the weather conditions. Another fact deduced was that within the above limits the differences of pressure between the maximum and minimum remained the same, whereas for the lower strata of atmosphere, between 1,800 m. and 400 m., the difference of pressure is 5 mm. greater for the maximum than for the minimum. From later observations the decrease of temperature with elevation was for the valleys 0.52° C. for 100 m., but was much greater, especially in winter, and more uniform between 1,900 m. and 3,100 m., averaging 0.68° per 100 m. and exceeding the normals previously calculated by Hann for the Alpine regions. The diurnal period of the sunshine at this and at other high stations shows a maximum in the morning, a decrease at noon, and a second slight increase late in the afternoon, which corresponds to the daily period of the humidity and the cloudiness. Dr. Hann shows that during the winter the pressure and temperature bear a direct relation to each other—with rising pressure the temperature also rises, and *vice versa*—while below no definite relation exists. Accordingly, during the winter, the temperature of the summit stations is chiefly dependent upon the pressure and incidentally upon the wind direction, etc.

THE STORM OF MARCH 11-14, 1888.

Investigated by the New England Meteorological Society, with the Aid of a Grant from the Trustees of the Elizabeth Thompson Science Fund.

BY WINSLOW UPTON.

The storm which visited the eastern part of the United States on the above dates was of exceptional interest, not only to the general public, but also to meteorologists. Its peculiar characteristics were—(1) the rapidity with which its energy was de-

veloped; (2) the excessive precipitation accompanying it, which was principally in the form of snow; (3) the fact that its maximum intensity was limited to a relatively small area. Fortunately for its study, the maximum area was situated in a region where there are many observers, and as this region was in southern New England, it became the duty of the New England Meteorological Society to investigate the phenomena which it exhibited. The following discussion is based upon more than five hundred reports. Of these, fifty-two were kindly furnished by Commander J. R. Bartlett, U. S. Hydrographer, and are reports from vessels off the coast; more than two hundred and fifty, relating principally to the snow-fall, were made by postmasters and others who responded to an appeal for such records; the remainder were furnished by the regular observers of the Society, including the thirteen observers of the U. S. Signal Service, who made full transcripts of their records, under instruction from the Chief Signal Officer. Many tracings of self-registering instruments were also kindly supplied. For all the above, grateful acknowledgment is made, as also to the Trustees of the Elizabeth Thompson Science Fund for financial aid in meeting the expenses of the investigation. The U. S. Signal Service has printed tri-daily maps from March 10th, 10 P. M., through March 15th, 3 P. M., which were available in revising for publication this paper. In its original form it was presented to the New England Meteorological Society at its regular meeting, April 17, 1888.

General History.—On the morning of the 10th inst., an elliptical depression extended from Arkansas northerly. The lowest pressure was 29.84 inches, at Green Bay, Wis., while a second centre, with pressure 29.86, existed near St. Louis. During the day the cyclone moved easterly, the southern centre tending more southeasterly than the northern. At 7 A. M. of the 11th, the two centres were well defined as separate cyclones, the northern in Ontario, with pressure below 29.7 inches, and the southern in Georgia, with lowest pressure 29.88. The precipitation was light snow or rain in connection with the northern cyclone, but in the South Atlantic States the passage of the southern

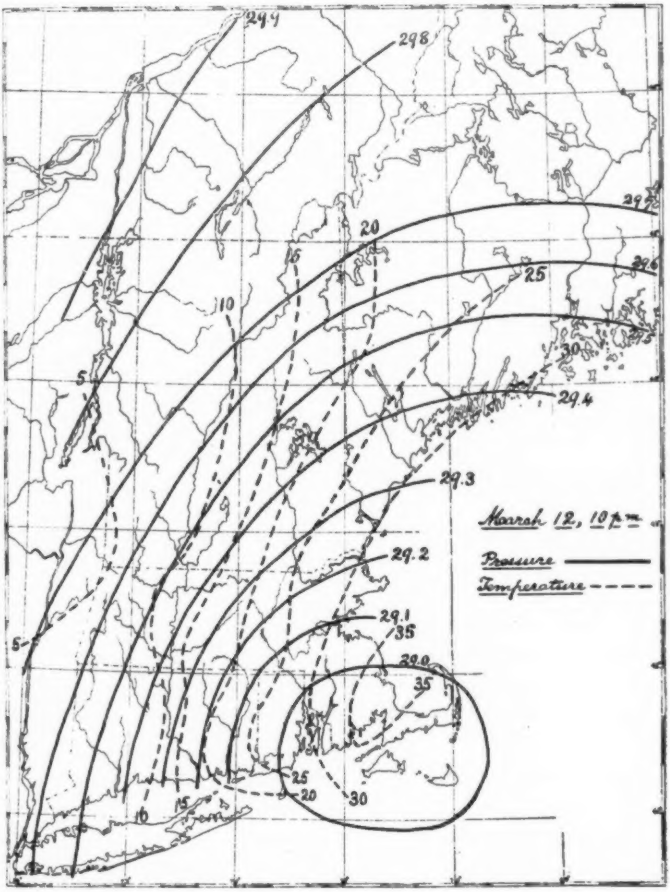


CHART I.—Isobars and Isotherms in New England, March 12, 1888, 10 P. M.

centre was attended by very heavy rains. Over four inches was recorded at Pensacola, Fla. During the 11th, the northern centre passed beyond the region of the reports, and a cold wave, with temperatures from 10° to 30° Fahr. lower than that prevailing during the passage of the cyclone, advanced over the Lake Region towards New England.

The southern cyclone now rapidly increased in intensity, and curving to the northeast, reached the coast near Wilmington, N. C., on the afternoon of the 11th. The pressure at the centre at this time was about 29.7 inches. Rain and snow began to fall in New England, and winds of increasing violence prevailed on the coast. In the next twenty-four hours the centre continued to move northeasterly in the Atlantic, with pressure falling, and at 10 P. M. of the 12th, when the centre was near the island of Nantucket, the lowest pressure was about 28.9 inches. At this point, however, it ceased to move in a northeasterly direction, and changing its course towards the west, advanced slowly over northern Rhode Island and Connecticut. At 7 A. M. of the 13th the centre was in Narragansett Bay, and at 10 P. M. near New London, Connecticut, but the cyclone had begun to lose its intensity. At 3 P. M. of the 13th the lowest pressure was 29.18, at New London, at 10 P. M. 29.44, and at 7 A. M. of the 14th the cyclone was no longer well defined, but the pressures 29.70 at Middletown, 29.68 at Providence, 29.67 at New Bedford, 29.70 at Wood's Holl and Block Island, marked the elliptical region in which it was losing its independent existence. At the same time a second centre was in formation in the Atlantic, east of Massachusetts Bay, which by the 17th inst. had advanced across the ocean as far as latitude 50°, and longitude 40° W. We have therefore in this storm an example of a well-developed cyclone changing its course and ceasing to exist over southern New England. Before seeking an explanation of these phenomena, it may be well to illustrate the facts already described, and to discuss more in detail the attendant phenomena.

Four charts are presented, which give the isobars and isotherms for the times (75th meridian) March 12th, 10 P. M.; 13th 7 A. M., 10 P. M.; 14th, 7 A. M. The data from which the maps have been constructed is given in the following table, which also contains the wind directions and velocities, for which there was no room on the charts. In drawing the isotherms, thermometer readings at seventy-eight other stations were employed, which it is thought unnecessary to reproduce. The isobars represent quite closely the barometric data; the isotherms are necessarily

more approximate on account of local variations in thermometer readings, but they may be considered as fairly representing the general distribution of the temperature at the hours given. At the stations in this list which do not read the barometer at 3 and 10 P. M., but at 2 and 9 P. M., interpolated values were found to agree with the system of isobars drawn.

TABLE I.

Station.	March 12, 10 P. M.				March 13, 10 P. M.			
	Pressure reduced to sea-level.	Air Temp.	Wind.		Pressure reduced to sea-level.	Air Temp.	Wind.	
			Veloc.	Direc			Veloc.	Direc.
Eastport, Me	29.72	30	56	E.	29.75	34	30	E.
Portland, Me	29.37	29	31	N E.	29.66	33	17	E.
Manchester, N. H	29.32	24	15	N.	29.62	30	7	N E.
Nashua, N. H.	29.30	26			29.61	27		
Brattleboro', Vt	29.51	9			29.64	28		
Northfield, Vt	29.76	11	32	N.	29.76	18	4	N.W.
Blue Hill, Mass	29.05	32	36	N.N.E	29.60	24	13	E.
Boston, Mass	29.09	32	36	N.	29.60	30	12	N.E.
Nantucket, Mass	28.93	33	12	S.	29.56	28	10	S.
Wood's Holl, Mass	28.92	35	18	N E.	29.55	27	6	E.
Block Island, R. I	29.00	20	42	N	29.52	26	24	S E.
Providence, R. I	28.98	29		N.	29.53	26		E.N.E.
New Haven, Conn	29 36	6	36	N.	29.52	23	20	N.
New London, Conn	29.11	25	26	N.	29.44	25	6	N E.
Albany, N. Y	29.77	5	18	W.	29.67	13	10	N.W.
New York, N. Y	29.64	11	34	W.	29.61	14	15	W.
Pilot Boat: Long. 70° 30', Lat. 40° 30'.	29.21			S.W.				

Station.	March 13, 7 A. M				March 14, 7 A. M.			
	Pressure reduced to sea-level.	Air Temp	Wind.		Pressure reduced to sea-level.	Air Temp.	Wind,	
			Veloc.	Direc.			Veloc.	Direc.
Eastport, Me	29.52	32	60	E.	29.86	31	18	N.E.
Gardiner, "	29.37	29		E.	29.93	34		N.E.
Portland, "	29.32	32	27	S.E.	29.82	34	15	N.
Manchester; N. H.	29.22	28	13	N.E.	29.79	32	14	N.E.
Nashua, "	29.20	26			29.78	31		
Brattleboro', Vt	29.31	12			29.81	30		
Northfield, "	29.54	6	28	N E.	29.92	21	2	S.
Blue Hill, Mass	29 11	22	30	E.	29.73	30	17	N.E.
Boston, "	29.15	24	38	N.E.	29.74	33	15	N.
Nantucket, "	29.08	21	27	S.	29.71	32	1	E.
New Bedford, Mass	28.91	24		N.E.	29.67	28		E.N.E.
Newburyport, "	29.20	28	22	E.	29.78	33	14	N.E.
Springfield, "	29.05	21		N.	29.72	29		N.
Taunton, "	29.04	22		N E.	29.71	31		
Wood's Holl, "	28.96	22	35	S.E.	29.70	30	8	N.E.
Block Island, R. I	28.92	24	36	N.	29.70	30	24	N.
Providence, "	29.00	22		N.N.E.	29.68	30		N.
Middletown, Conn	29.21	6		N.W.	29.70	24		N.E.
New Haven, "	29.23	7		N.W.	29.74	26	9	N.E.
New London, "	29.05	14	28	N.	29.71	27	4	N.E.
Albany, N. Y	29.58	2	14	N.W,	29.85	20	2	N.
New York, N. Y	29.45	6	37	W.	29.75	23	14	N.W.

Special Characteristics. 1. *Wind.*—The table already given shows the wind velocities at the hours of observation. The maximum velocities recorded are given in the following table, together with the times at which the velocity was greatest. It will be noted that these times are on the 12th or early on the 13th, when the storm centre was approaching the New England coast. It will be observed, also, that while the greatest velocities were recorded at the stations on the coast, the velocities farther inland are worthy of remark.

TABLE II.

Maximum Wind Velocity.

Station.	Time.	Velocity.	Direction.
New York	13, 3:32 A. M.	50	
New Haven	12, 3:25 P. M.	60	N.
New London	12, P. M.	46	
Nantucket	12, 4:30 P. M.	54	S. E.
Block Island	12, 11:00 A.M.	70	
Blue Hill	12, 5-6 P. M.	57	
Boston	13, 4:05 / 7:30 A. M.	60	N. E.
Provincetown	12, 5:00 P. M.	65	N. N. E.
Portland	12, 10:02 P. M.	42	N. E.
Eastport	13, 6:18 A.M.	72	
Nashua	13,	25	N. E.
Manchester, N. H.	12, 9:30 P. M.	28	N. E.
	13, 9:20 A. M.	30	
Albany	13, 3:45 P. M.		
Northfield	12, 6:46 P. M.	40	N.

The direction of the wind at the several stations corresponds, with scarcely an exception, with the usual law of the winds about a centre of low pressure, and this is found also to be true in the observations made at many other stations in the several States, which are not here reproduced. Naturally, winds of such intensity caused great damage along the coast, resulting in many wrecks and in considerable loss of life, as the direction of the wind was towards the shore.

2. *Precipitation.*—The precipitation was excessive, partly snow and partly rain; west of the 72d meridian it was almost wholly snow, and piled up in immense drifts, making it absolutely impossible to measure its amount with even approximate

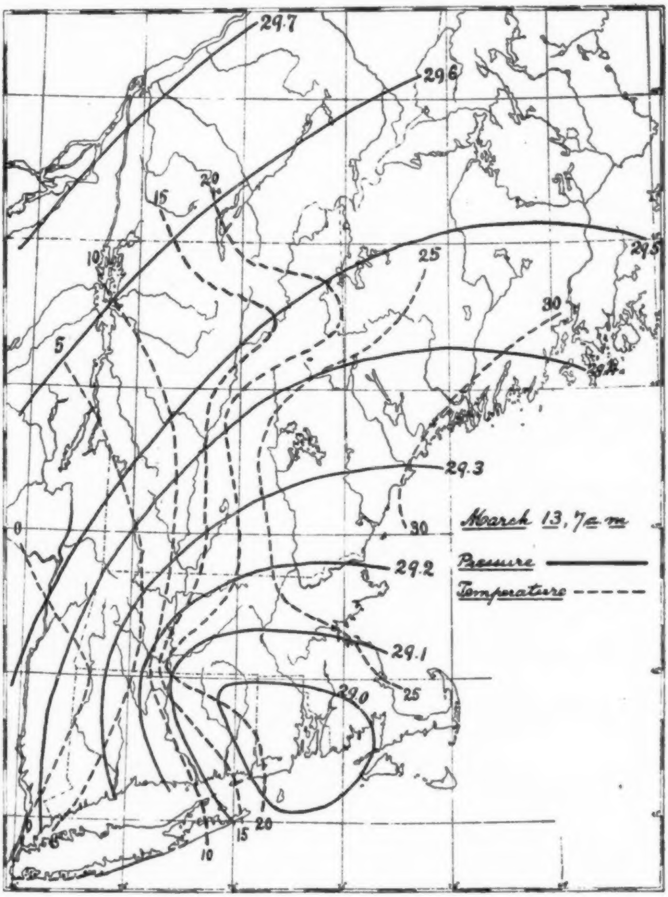


CHART II.—Isobars and Isotherms in New England, March 13, 1888, 7 A. M.

accuracy. East of this meridian it was snow and rain mixed, and in southeastern Massachusetts almost wholly rain. Any attempt to determine the distribution of precipitation under such circumstances is necessarily unsatisfactory, and leads to only approximate results. As gauges are useless, recourse must be had to estimated depths of unmelted snow, which, if care-

fully made, and then sections obtained in a place or places of average depth, and the sections melted or weighed, will give results of some value. The chief difficulty is in deciding upon the spot to be taken as a place of average depth. In order to obtain the distribution as well as possible, a circular letter was prepared and widely distributed, asking for careful estimates of

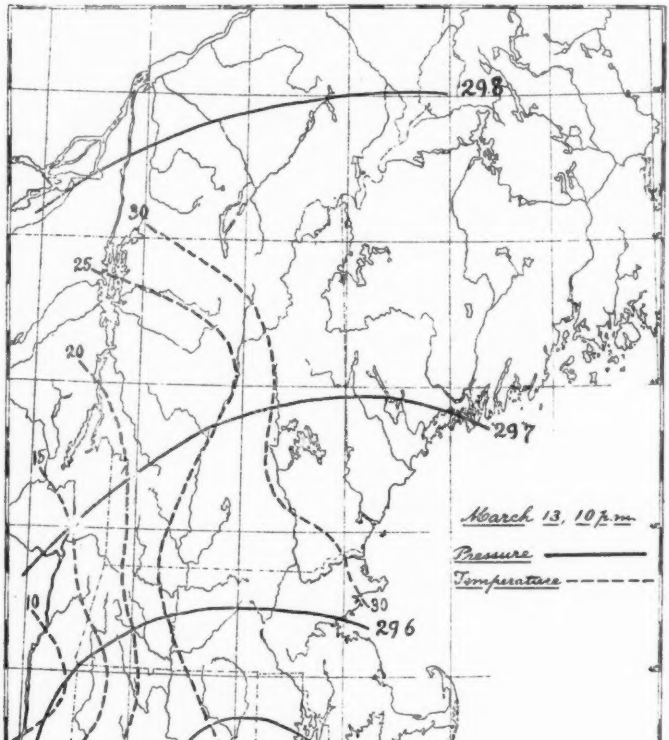


CHART III.—Isobars and Isotherms in New England, March 13, 1888, 10 P. M.

the average depth of the snow-fall, together with other observations made during the storm. More than three hundred replies were received, and these, added to the reports of the regular observers, gave a total of 425 estimates of the snow-fall. The determinations of the amount of melted snow and rain were m ade by regular observers only, and number 110.

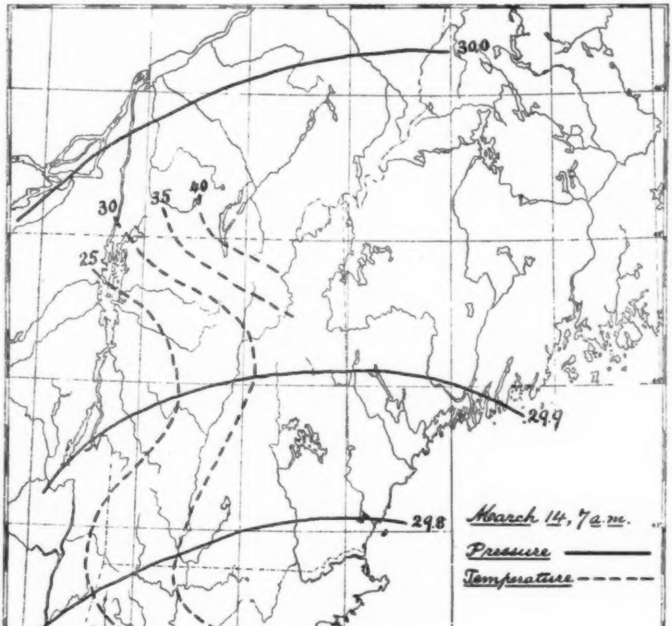


CHART IV.—Isobars and Isotherms in New England, March 14, 1888, 7 A. M.

Table III (pp. 30–34) contains the estimates of the snow-fall and the total precipitation, and is given in full, in order to show the data available, and also to give an idea of the trustworthiness of the conclusions which may be derived therefrom. Undoubtedly casual observers overestimate the depth of snow. This can be avoided by making actual measurements, in which case the selection of a spot of average depth is the greatest difficulty. The latter difficulty is reduced by making the measurements in wooded territory, if such exists. A large part of the estimates given below were made by actual measurement, and in many instances with considerable care. Of these the following are known to have been made in woods: Gorham, N. H.; Londonderry, Marlboro'. Vt.; New Salem, Rowe, Mass.; Ellington, Enfield, New Milford, Plainfield, Wallingford, West Hartford, Conn.; Westbury, L. I.

In order to represent the precipitation graphically, the accompanying chart is given. It contains lines of equal snow-fall, drawn from the estimates of average depth given in the preceding table, and also the isohyetal lines for the eastern part of the country, in which the precipitation was either rain alone or rain and snow mixed. The lines of equal snow-fall, (in fact those representing the total precipitation also,) must be considered as only approximate, but considerable confidence is placed in them because (1) there is good agreement in the independent estimates of neighboring observers, (2) the measurements in woods give values according well with the others in their vicinity, (3) in the few cases where the snow was melted, as at Albany, Hartford, Middletown, New Haven, and Setauket, the amounts obtained were also in accordance with the distribution as charted. It will be seen from the chart that in the region embracing nearly the southern half of Vermont and of New Hampshire west of the Merrimac, the western half of Massachusetts, nearly the whole of Connecticut and of New York east of the Hudson as far north as Lake George, the average depth of unmelted snow exceeded 30 inches, while in central Connecticut and a large part of eastern New York the average fall was over 40 inches. Within this area, there seems to have been a region

near the Connecticut river in Massachusetts where the fall was a little less than 30 inches, and a more marked region in the vicinity of Hartford, Connecticut, where the fall was less than 20 inches. In Rhode Island and eastern Massachusetts, where the precipitation was snow and rain mixed, the amount of rain was excessive, as indicated by the dotted lines on the map.

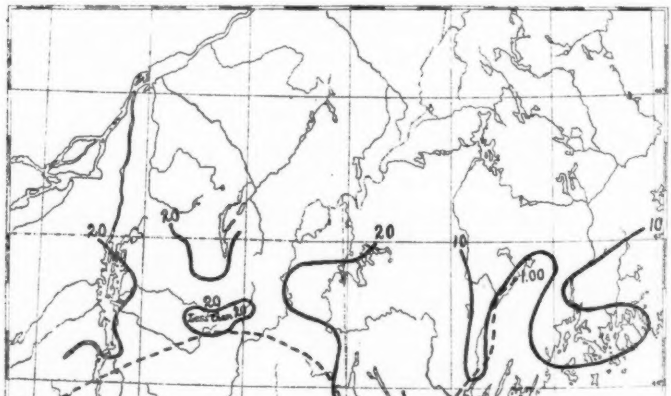


CHART V.—Distribution of Precipitation in New England, March 11-14, 1888.

TABLE III.

Average Depth of Unmelted Snow, and Total Precipitation.

Station.	Est. Snow.	Total Precip.
Eastern New York.		
Albany	47	3.63
Altramont	40	
Ballston	50	
Bangall	40	
Berlin	38	
Bolton	30	
Carmel	30	
Chatham	42	
Claverack	48	
Claverack	34	
Cohoos	36	
Cold Spring	48	
Coxsackie	48	
Croton Falls	36	
Dover Plains	39	
Edinburgh	32	
Elizabethtown	15	
Hoosick Falls	42	
Hudson	48	
Lebanon Springs	38	
Mamaroneck	24	
Middle Granville	48	
Millerton	48	
Mt. Vernon	36	
New Rochelle	23	
New York	22	2.46
Palenville	24	
Patterson	19	
Pawling	24	
Peekskill	26	
Pine Plains	30	
Poughquag	30	
Plattsburg	8	
Rhinebeck	49	
Rockville	36	
Sag Harbor	18	
Sandy Hill	52	
Saratoga	50	
Schaghticoke	49	
Sharon Station	20	
Stapleton, S. I.	24	
Tarrytown	24	
Towner's	20	
Troy	55	
Troy	46	
Valatie	48	
Valatie	42	
Verbank	28	
Westport	24	
White Plains	32	
Long Island.		
Babylon	36	
Bridge Hampton	10	
Flushing	32	
Glen Cove	33	
Greenport	18	
Islip	29	
Newtown	10	
Patchogue	33	
Peconic	18	
Setauket	24	
Smithtown	24	
Speonk	30	
Springfield	20	
Westbury	21	
Connecticut.		
Ashford	36	
Barkhampstead	30	
Branford	36	
Bridgeport	18	
Bristol	38	
Brooklyn	42	
Canterbury	14	
Canton	24	1.92
Cheshire	40	
Colchester	36	
Colebrook	36	
Collinsville	37	
Coventry	42	
Ellington	36	
Enfield	36	
Fairfield	18	
Falls Village	34	
Franklin	24	
Goshen	43	
Granby	27	
Groton	12	
Guildford	44	
Haddam	24	
Hamden	42	
Hartford	19	2.69
Higganum	42	
Killingly	28	
Killingworth	40	
Lake Konomoc		2.17
Lebanon	36	
Litchfield	38	
Lyme	24	
Marlborough	48	

TABLE III.—*Continued.*

Station.	Est. Snow.	Total Precip.
Middletown	50	5.78
Milford	24	
New Fairfield	39	
New Hartford	42	
New Haven	42	4.50
New Milford	27	
Newtown	24	
New London	20	2.17
North Stonington	20	
Norwalk	20	
Norwich	24	
Plainfield	20	
Plymouth	36	
Pomfret	24	
Portland	36	
Ridgefield	19	
Salem	19	
Saybrook	36	
Scotland	20	
Shelton	25	
Southbury	36	2.27
Southington	21	
South Manchester	36	
Southport	36	
Thomaston	42	
Thompson	36	
Tolland	36	
Uncasville	36	
Vernon	39	3.26
Voluntown		
Wallingford	36	3.70
Waterbury	42	
Weston	18	3.55
Westport	25	
Westport	36	
Willington	30	
Woodbury	25	
Woodbury	30	
Woodbury	39	
Rhode Island.		
Block Island	6	1.00
East Greenwich	6	
Harrisville	18	
Hopkinton	15	
Kingston	20	
Little Compton	1	
Lonsdale	6	2.35
Narragansett Pier	8	
Olneyville	6	
Pawtucket	8	1.77
Providence	8	2.86
Providence	8	2.15
Westerly	10	
Woonsocket	16	1.85
Massachusetts.		
Amherst	16	3.35
Ashburnham	36	
Becket	36	
Beverly Farms	14	2.64
Blue Hill	9	3.07
Boston	12	2.81
Buckland	30	
Buckland	32	
Cambridge	12	2.34
Cambridge		2.37
Cheshire	24	
Chestnut Hill		2.59
Chicopee	36	
Chicopee Falls	36	
Clinton		1.60
Concord	24	2.85
Cotuit	0	0.89
Cummington	39	
Deerfield	24	3.20
Deerfield	27	
Dudley	36	3.50
Dudley	30	
Easthampton	34	
Fall River		2.86
Fiskdale	36	
Fitchburg	25	2.74
Fitchburg	25	2.38
Framingham		3.33
Gilbertville	36	4.00
Granville	36	
Greenfield	34	
Groton	14	
Groton	31	3.52
Groton	13	
Hinsdale	35	
Holden	26	
Lake Cochituate		2.77
Lawrence	14	
Lawrence	16	2.16
Leicester	23	
Lenox	29	
Leominster	18	2.77
Leominster	18	2.90
Long Plain		2.45
Lowell	20	
Lowell	18	3.70
Ludlow	24	2.90

TABLE III.—*Continued.*

Station.	Est. Snow.	Total Precip.
Lynn	10	2.95
Mansfield	7	3.26
Mansfield	38	3.16
Marlborough	15	
Medford		1.80
Middleboro	1	2.00
Milton	7	3.30
Monson	39	4.25
Mt. Nonotuck	32	3.65
Mystic Lake		2.31
Mystic Station		2.20
Nantucket	2	0.68
New Ashford	36	
New Bedford		1.45
New Bedford	1	1.42
New Braintree	30	
Newburyport	16	3.35
New Salem	24	
New Salem	25	
Newton		2.09
North Adams	40	
Northampton	34	4.68
Norwich	36	
Oxford	30	
Petersham	36	
Pittsfield	36	
Plymouth		1.30
Princeton	30	
Provincetown	1	0.87
Rowe	28	3.40
Salem	8	2.28
South Egremont	30	
South Hadley	33	
South Hingham	2	2.10
Springfield	37	3.60
Stockbridge	36	
Taunton	1	2.65
Taunton		1.23
Taunton		2.48
Townsend	25	
Uxbridge	20	
Waltham		2.51
Warren	36	
Warwick	36	
Webster	32	
Wellesley		3.57
Westboro	24	3.00
Westfield	30	
Winchester	12	1.92
Windsor	34	
Worcester	30	
Wood's Holl	0	1.27
Vermont.		
Alburgh Springs	23	
Arlington	36	
Barton	18	
Bellows Falls	32	
Bennington	48	
Brattleboro	40	
Brunswick	24	
Burlington	23	1.15
Chelsea	14	1.05
Chester	36	
Cornwall	24	2.40
Danby	36	
Danville	12	
Derby	20	
East Charleston	24	
East Dorset	26	
Enosburgh Falls	22	
Fairfax	22	
Fletcher	24	
Glover	29	
Greensboro	22	
Jacksonville	40	4.13
Jericho	18	
Leicester	18	
Lemington	24	
Londonderry	36	
Lowell	14	
Lunenburgh	24	2.40
Lyndonville	18	
Manchester	24	2.40
Marlboro	40	3.61
Marshfield	22	
Middlebury	19	
Milton	20	
Montpelier	22	
Newbury	26	
New Haven	30	
Newport	18	
Northfield	27	2.22
Norwich	25	
Peacham	22	
Peru	33	
Pittsford	22	
Plymouth	36	
Poultney	30	
Richford	15	
Rutland	24	
Ryegate	24	
St. Johnsbury	27	
So. Shaftsbury	42	
Strafford	20	2.00

TABLE III.—*Continued.*

Station.	Est. Snow.	Total Precip.
Stratton	42	
Summerville	24	
Vergennes	20	
Vernon	38	4.35
Underhill Centre	23	
Warren	20	
West Fairlee	24	
West Randolph	28	
Woodbury	15	
New Hampshire.		
Ackworth	33	
Allenstown	24	
Alton Bay	18	
Amherst	36	
Amoskeag Falls	26	
Amoskeag Falls	27	
Antrim	30	
Atkinson	24	
Atkinson	30	
Bartlett	26	
Berlin Mills	24	1.85
Bethlehem	23	
Boscawen	24	
Bradford	28	
Campton Village	20	
Canterbury	22	
Chesterfield	40	3.85
Claremont	32	
Claremont	36	
Colebrook	24	
Concord	27	2.56
Contoocook	24	2.00
Derry	20	
Dublin	42	
Enfield	30	
Exeter	18	3.21
Farmington	24	
Fitzwilliam	36	
Goffstown	22	
Gorham	22	
Grafton	30	3.00
Great Falls	21	
Hanover	25	3.00
Haverhill	20	
Hillsborough	30	
Hopkinton	24	
Hookset	20	
Jefferson	24	
Keene	36	
Keene	30	
Laconia	31	
Lake Village		3.27
Lebanon	24	
Littleton	18	
Manchester	24	2.40
Manchester	23	2.31
Manchester	21	2.05
Meriden	36	
Milton Mills	16	
Nashua	30	3.05
New Boston	27	
New Hampton	32	
New Ipswich	34	
New London	24	
New London	36	
North Conway	15	2.00
Ossipee	24	
Pittsfield	24	
Plymouth	26	4.25
Raymond	18	
Rochester	22	
Rumney	23	
Shelburne	20	1.98
Stratford	12	1.09
Suncook	18	
Tamworth	30	
Troy	36	
Walpole	30	
Walpole	28	2.50
Washington	36	
Weare	28	
West Milan	17	
West Salisbury	24	
Maine.		
Andover	14	
Andover	18	
Andover	20	
Auburn	10	
Augusta	8	
Aurora	18	
Bar Harbor	7	0.66
Belfast	6	
Belgrade	8	
Biddeford	18	
Bingham	10	
Boothby	20	
Bridgton	12	
Buckfield	12	
Bucksport	14	
Calais	13	
Cambridge	8	
Camden	5	

TABLE III.—*Concluded.*

Station.	Est. Snow.	Total Precip.	Station.	Est. Snow.	Total Precip.
Castine	6		Monticello	10	
Castine	4		Norridgewock	8	
Corinna	18		Oldtown	5	
Dexter	4		Orneville	8	
Eastport	4	0.35	Poland	18	
Etna	15		Portland	13	0.69
Fairfield		0.35	Richmond	8	
Fryeburg	15		Rockland	6	
Gardiner	8	1.00	St. Albans	8	
Gorham	10		Scarboro'	15	
Greenville	7		Searsport	4	
Houlton	8		Skowhegan	9	1.80
Jonesboro	12		Thomaston	12	
Kennebunk	14		Turner	12	
Kent's Hill	12	1.50	Unity	12	
Lewiston	14	1.43	Vassalboro	16	
Lisbon	15		Waterville	10	
Lubec	18		Wesley	16	
Lubec	9		West Falmouth	12	
Machias	12		Whitneyville	12	
Mechanics Falls	16		York	18	
Medford	6		*New Brunswick.*		
Mercer	12				
Monson	8		St. John	4	0.85

Some of the snow-drifts which were actually measured were of astonishing height. In Bangall, Dutchess county, New York, the measurements gave heights from fifteen to forty feet, and in Cheshire, New Haven county, Connecticut, one of thirty-eight feet was measured. The maximum precipitation reported was at Middletown, Connecticut, 5.78 inches.

The snow was very dense along the coast, but further inland was of ordinary density. A section of snow taken at Providence, upon which, however, much rain had fallen, yielded water in the proportion 1 inch snow to 0.480 inch water. The following table of results obtained by different observers is given. It is classified in two parts, according as the result was obtained by melting a section of snow obtained by inverting the gauge in a place of average depth, or by simply melting the snow collected in the gauge. In a high wind the former method, though approximate, is generally more accurate than the latter.

TABLE IV.

Ratio of Unmelted to Melted Snow.

By Section.		By Gauge.	
Station.	Ratio.	Station.	Ratio.
Providence	0.480	Lynn	0.290
Pawtucket	0.226	Amherst	0.209
Newburyport	0.216	Cambridge	0.197
Leominster	0.161	Lowell	0.185
Hartford	0.144	Beverly Farms	0.182
Lawrence	0.135	Northampton	0.138
Gilbertville	0.111	Woonsocket	0.116
Nashua	0.107	Middletown	0.116
Manchester, N. H	0.100	Groton	0.114
Fitchburg	0.100	Albany	0.078
		Gardiner	0.075

From the above it will be seen that the density of the snow varied greatly, even at stations where no rain had fallen together with the snow. This storm, therefore, with many preceding storms, furnishes proof that the method of assuming that 1 inch of snow equals 0.10 inch water—a method which unfortunately is still in use by many observers—is exceedingly erroneous. In no storm is it safe to assume this ratio, and the method ought to be forever discarded by observers.

3. *Temperature.*—Referring to the history of the cyclone given in the early part of this paper, it will be remembered that the cyclone under discussion was originally the southern wing of a depression whose principal centre moved from Wisconsin easterly over the Lakes, and then passed into Canada, or else was dissipated on the afternoon of the 11th. It was followed by a cold wave, with temperatures about 30° Fahr. lower than those prevailing during the passage of the cyclone. This area of low temperature reached the western boundary of New England on the 12th and 13th, just as the other cyclone was advancing towards its southern boundary.

On March 11th the temperatures over New England ranged between 30° and 40° Fahr. On the 12th the temperature began to fall on the western boundary, and continued to fall, the cold

wave progressing easterly, while the storm was raging. An examination of the isotherms given on charts I–IV will show its subsequent history. Chart I shows the distribution of temperature as the cold area was advancing, but with its crest not yet within the limits of the map. Chart II shows the isotherm 0° on the western boundary, but east of the 72d meridian the temperature had ceased to fall, and there was a steep temperature gradient in western Massachusetts and Connecticut. Chart III shows that the check in the advance of the cold area was permanent, and that the rise of temperature had extended over the whole area of the map, which is further shown by Chart IV. The area of low temperature then ceased to advance, and was dissipated simultaneously with the cyclone itself.

We have in the history of the temperatures thus outlined an explanation of several features of the storm. Observers in southeastern Massachusetts commented upon the unusual fact that on the 12th, with a strong southerly wind, the temperature fell. This fall was due to the advancing cold area, and the prevalence of southerly winds was due to the cyclonic circulation. Usually as a cold wave advances, anti-cyclonic conditions prevail, as was the case in this storm west of New England.

We have also in the temperature history an explanation of the excessive precipitation, and also of its geographical location. The fall was heaviest on the 12th, as indicated by the following table, which contains a few selected reports only.

TABLE V.

Precipitation at U. S. S. S. Stations near Maximum Area.

Station.	Before 11th, 10 P. M.	11th, 10 P. M. — 12th, 7 A. M.	12th.		12th, 10 P. M. — 13th, 7 A. M.	After 13th, 7 A. M.
			7 A. M. — 3 P. M.	3 P. M. — 10 P.M.		
Albany	0.28	0 85	0.70	0.74	0.30	0.76
New York	0.40	0.96	0.54	0.08	0.32	0.16
New Haven	0.07	0.90	1.10	0.80	0.63	1.00
New London	0.03	0.52	I.24	0.12	0.06	0.20
Block Island	0.02	0.04	0.04	0.80	0.02	0.08
Wood's Holl	0.00	0.74	0.43	0.00	0.00	0.10

The time of heaviest precipitation coincides closely with the time of the cooling of the air, already fully saturated. If air which is saturated with aqueous vapor at temperature 32° is cooled 20°, it will hold only about one-half its vapor. The cyclone was one attended by heavy precipitation, as was shown by the excessive rain-fall in the southern states. When advancing up the coast the attendant precipitation was from two to three inches. The increase of precipitation due to the cooling of the air by 20° would double this amount, and this is approximately what was observed in the region where the precipitation was greatest.

The arrival of the cold area from the west is also probably the cause of the deflection of the cyclone from its original path. Other instances are on record of such an occurrence, which is relatively rare but not previously unknown. In this case the deflection exceeded 90°, and the cyclone was drawn directly towards the cold area. The cyclone and cold wave thus met in mortal combat, and in this tragic duel both combatants came to an untimely end.

It has been conjectured in the course of this investigation that the depression which came into New England was not the principal development of the cyclone, but an off-shoot, the main centre continuing its northeasterly march. The data collected by the U. S. Hydrographic Office, however, when joined to those on land, seem to show that the principal centre actually reached the New England coast and was dissipated as above described, but that on the 14th a new centre was formed in the ocean, which continued in a northeasterly direction over the Atlantic.

While the excessive precipitation in this storm was its most significant feature, it has been surpassed on other occasions. In February, 1886, as described in this JOURNAL for January, 1887, a heavy rain-fall, reaching eight inches at the maximum, occurred in southern New England. The great prominence given to the storm under consideration was due to the fact that the form of the precipitation was snow, and that it fell in the vicinity of the city of New York, causing almost a complete suspension for several days of railway traffic centering in that city.

CORRESPONDENCE.

LOCAL WEATHER PREDICTIONS.

To the Editors.—In closing my part in this discussion, in which my position has been made sufficiently clear, I think, I desire to set Mr. Clayton right on one or two points, and call attention to the main question, which I fear will be covered up by so many words. In my original article (December, 1887) I tried to give a brief statement of what had been done, and distinctly mentioned the Bulletin of the N. E. Meteorological Society as my authority. Any one who will examine this will see that I have been borne out in all my work by that. I find month after month, November, 1886, to July, 1887, a statement that the Blue Hill predictions were made at sunset.

Mr. Clayton surprisingly calls this a "clerical error." It is hardly to be expected he will admit that I have been perfectly justified in all my statements based on this data as published.

Again I tried to show what a "skillfully ignorant" person could do as compared with the work of Mr. Clayton, beginning with May, 1887, when, as I supposed, the first comparisons with the Signal Service according to Signal Service methods began. I took all the months published, and it seems to me that all this talk since has not touched in any way my original position, nor do I think that it will serve to cover up or shake it. The only thing that was necessary was to show that my figures were improperly obtained, which has not been done; that other months show different results does not affect the main question in the least. I have tried to show:

1st. During several months of 1887, a prediction by an almanac maker 100 years in advance would have gained a higher per cent. than that of Mr. Clayton; this has not been denied.

2nd. That in a dry time when "all signs of rain fail" a skillful person ought to obtain a higher percentage than a "skillfully ignorant" one. In the driest month of 1887 Mr. Clayton made his lowest percentage. I do not comprehend just his position at present.

3rd. That when two persons were pinned down to definite predictions for a definite locality, and these were verified by Mr. Clayton's own rules; the one on the spot made 80 per cent., and the other, 300 miles away, 96 per cent.; this has not been denied.

4th. That a prediction 34 hours in advance is practically a guess. A careful analysis of Mr. Clayton's predictions and the resulting weather will abundantly prove this. There are times when we are sure of the weather, and it does not require any skill to predict it, but at times it is more often a guess. As just shown, Mr. C. made his lowest per cent. in a month when, if we are to believe his latest utterance, he should have made the highest. I have advanced other proofs of this, and I now emphatically maintain my position.

One word more. In my first article I mildly objected to the methods of Mr. Clayton in comparing utterly dissimilar things. The justice of this is now clearly shown by the fact that Mr. Clayton feels himself forced to abandon his former position, and he now proposes to change his rules of verification. This is a great mistake, and Mr. Clayton should understand that he must take back all he has done, or continue to confine himself to the rules of his own choosing. Mr. Clayton has set himself up as a judge, he has published broadcast to the world that, presumably by fair comparisons, he has obtained from 5 to 10 per cent. higher values than the government. He has insisted on applying his own rules and on interpreting his own and other predictions, notwithstanding the fact that the Signal Service published its method of verification in its Annual Report for 1885, and in two places in the Report of the Joint Commission issued 1886. I do not see that there is any occasion whatever for Mr. Clayton's going to those rules at this late day. It appears very plain that on being forced to make this change he admits the unfairness of his former comparisons. His ignorance of these publications is no excuse for his serious attempts at discrediting the work of the government bureau; moreover, he could have ascertained without difficulty that he was unjustified in his strictures because his verifications were often 20 to 25 per cent. lower than those of the government for the same

predictions. Mr. Clayton strangely fails to see that the principal error in his comparisons has arisen from *a false interpretation of the wording of the government prediction.*

How unjust his comparisons were is shown by an article in *Science* for February 10. Mr. Clayton, interpreting his own predictions, gave himself in October 85 per cent., applying precisely the same rules, and interpreting according to his own views he gave the government predictions 58 per cent., a difference of 27 per cent. in his favor.

Mr. Clayton admits (*Sicence,* Feb. 3, 1888) that Mr. Hazen made the government predictions as well as others for Boston during October, 1887. These predictions, then, are directly comparable, at least no one would think that the same person would make different predictions for the same day except as the wording might be slightly varied to suit the circumstances. The predictions of Mr. Hazen, when verified by Mr. Clayton's rules, gave 96 per cent. This enormous difference of 38 per cent. shows how little reliance can be placed upon Mr. Clayton's comparisons.

I think it is very plain that until Mr. Clayton can explain this great difference he should be a little cautious, to say the least, in sowing his views broadcast in this country and in Europe. I most sincerely trust that others may be led to take up this discussion.

Mr. Clayton should be more careful in his statement of facts. He declares that the verifications of his 16 hour predictions were not thought worthy of publication, and yet they were repeatedly published in the N. E. Bulletin. It may seem to some that my language has been a little harsh, but I believe if any one will carefully read over this discussion, he will find that Mr. Clayton has imputed to me an unfairness that is entirely devoid of truth, and yet he will not acknowledge his error. GAN.

To the Editors.—Most of the above letter of "Gan" has been sufficiently answered in the letters on this subject which have preceded this; but there are two points in this to which it seems necessary to reply, in order to prevent a misunderstand-

ing by the reader. "Gan" says that I now admit that my former method of verification was unfair by now changing my method. I think, however, that he is entirely mistaken. The rules which have heretofore been used for verifying the Blue Hill and Signal Service predictions were rules made up by the Signal Service, and sent out to voluntary observers (myself among the number) for verifying the Signal Service predictions. How, then, it could be considered unfair to verify the Signal Service predictions by these rules, and compare them with others made under the same definitions and verified by the same rules, I am unable to realize.

So far as I know, these rules were the only ones published by the Signal Service until within the last three months, when the Chief Signal Officer's Report for 1886, and the Signal Service Monthly Weather Review for October, 1887, have appeared, containing the official rules for verification.

Since the former method used at Blue Hill was called in question, it was concluded, beginning with the present year, to verify the Signal Service predictions as nearly as possible by their official rules; and to verify the Blue Hill predictions in the same manner. The only change made was to adapt the rules to one station instead of apply them to areas. These rules are only a modification of the former rules used at Blue Hill; there is no change in definitions. According to the old rules, if rain was predicted and .01 inch or more fell at any time during the day, the prediction was considered fully verified; but according to the new rules rain must fall within each of at least two intervals of eight hours after the beginning of the prediction, or the prediction will only be partially verified. According to the old rules, if fair weather followed by rain was predicted, and .01 inch fell at any time during the day, the prediction was called fully verified; but according to the new rules no precipitation must fall during the first eight hours following the time for which the prediction was made to begin, or it will only be considered partially verified, etc. The new rules are only an attempt to apply a more rigid test to the predictions, though in this I think they fail, as I hope to show at some time.

In comparing the Blue Hill predictions with those of the Signal Service we have been very desirous of making the comparison as fair as possible; and for fear some injustice might be done them by using the old rules, we recently obtained the official Signal Service rules with all the modifications up to the present time, and a detailed explanation of the meaning of the terms used. With these rules we have verified both our own and the Signal Service predictions since the first of January. The following table shows the results up to the present time. All these predictions were published in the newspapers of southeastern New England except those made on Sunday:

	Jan.	Feb.	Mar.	Apr. (17 days)	Mean.
Blue Hill, 1:30 P. M. Predictions for 24 hours from midnight. Per cent. verified	88	83	84	87	85
Signal Service, 10:30 A. M. Indications for 24 hours from 3 P. M. Per cent. verified	75	91	83	85	83

The writer was absent from the observatory during most of February, and the predictions during this time were made by Mr. Rotch. It is seen from this table that during three months out of nearly four, the Blue Hill predictions were given a higher per cent. of success than those of the Signal Service, and have averaged two per cent. higher, notwithstanding that they are made for so much longer in advance, and during ten days in March, when telegraphic communication was cut off, were based entirely on local observations. This hardly seems to maintain the position which "Gan" claims to have proven, viz.: "that a prediction 34 hours in advance is practically a guess."

In the second place, "Gan" thinks that but little reliance can be put on the accuracy of the Blue Hill verifications. He says that for October we gave the Signal Service only 58 per cent., while some private predictions of Professor Hazen, when verified by our method, gave 96 per cent. These predictions for which "Gan" claims a verification of 96 per cent. were made to begin at midnight, and made in accordance with a set of rules differing from the Signal Service rules; and yet he says these

predictions are directly comparable with the government predictions, worded entirely differently and made to begin at 3 P. M. This seems a very slender basis on which to make such an accusation as that made by "Gan."

In my last letter on this subject, when referring to the Blue Hill morning predictions, I said,—"we consider them of so much less relative importance than the predictions of longer range that we *do* not publish the per cent. of verification,"—not that we had never published them. Perhaps I would have made my meaning clearer if I had said we have not published them for many months in the statements of the predictions sent out in our monthly summaries.

"Gan" insists on believing that I have misrepresented the Signal Service; but, if I have misrepresented them in any way, it was certainly unintentional. I fully appreciate their great value; my only desire is to make it more efficient than at present. I am convinced that more localized predictions and civilian, rather than military control, would increase the efficiency of the service, and until I am convinced to the contrary I shall maintain these convictions with all the ardor of my nature.

I regret very much that "Gan" should think that I am interested in undermining the character of a "nom de plume," and should after exhausting his arguments resort to accusation and abuse. H. HELM CLAYTON.

BLUE HILL OBSERVATORY, April 18, 1888.

POSITION OF A TORNADO IN A GENERAL STORM.

To the Editors.—I regret very much that I did not use a little more explicit language in referring to this subject on page 584 of the April number, as it has given rise to a misunderstanding on the part of Mr. Finley. In speaking of the first mention of this fact, I did not mean the first mention of *any* kind; and this is plainly indicated by my reference to the full discussion by Professor Davis in the August, 1884, JOURNAL.

What I did mean was the first definite mention and emphatic calling attention to the importance of this fact in theories of tor-

nado formation. I was familiar with all the Signal Office publications on this subject. I knew that in the annual report of the Chief Signal Officer for 1875, at page 398, there were given tracks of tornadoes, and two pages later the accompanying low area. The descriptions of this and other tornadoes in these reports showed that the general fact was recognized. Moreover we have evidence of the knowledge of this fact in the expression "conditions favorable to the development of tornadoes exist," etc.; also in that severe local storms were predicted from this knowledge.

The most extraordinary point in this connection is that Mr. Finley can find any correspondence between my views and his. The most powerful argument that Mr. Finley has used and reiterated is that tornadoes occur in the region where warm southerly winds meet cooler northerly, thus producing very great contrasts of temperature. While I have maintained as forcibly as I could that tornadoes occur to the southeast of a storm centre from 300 to 600 miles away and where there are absolutely no northerly winds and no contrasts of temperature, I am not in a position to enter a controversy on this question. Both Mr. Finley and I have published our views in scientific journals, and others must make the comparisons. I know that our views on this question are directly contradictory, but others must decide as to which corresponds with the facts.

H. A. Hazen.

CURRENT NOTES.

Sacramento (Cal.) Notes.—An unusual occurrence was the fall of two and a half inches of snow in the Sacramento valley on January 4th and 5th. The surface temperature did not reach frost, and no damage to vegetation is anticipated. The phenomenon was not entirely unprecedented, as shown by the following list from the Sacramento *Bee*, which we owe to Sergeant Barwick:

Snow at Sacramento since 1861.

January 29, 1862, three-quarters of an inch.
January 12, 1868, 1.62 inches.
December 3, 1873, 6 inches.
April 5, 1875, a trace; enough to whiten the ground.
January 13, 1879, a light trace.

January 26, 1880, estimated at about one-quarter of an inch, but it melted as fast as it fell.

February 17 and 18, 1882, light traces of snow.

December 31, 1882, about 4 inches.

February 1 and 6, 1883, a very light fall.

January is, however, a relatively wet month. The distribution of the rainfall at Sacramento is a characteristically a Californian one. The following table is clipped from the *Record-Union.* It extends from September, 1849, to January, 1888, and was collated from the records of Dr. T. M. Logan, Dr. F. W. Hatch, and those of the United States Signal Service, and tabulated by Sergeant James A. Barwick, Signal Service Observer.

Rainfall at Sacramento, Cal.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total for year.	Season of	Inches.
1849	....	....	....	....	....	....	....	....	.25	1.50	2.25	12.50	....	1849-50	36.00
1850	4.50	.50	10.00	4.25	.25	none	none	none	none	none	sprin	sprin	19.50	1850-51	4.71
1851	.65	.35	1.88	1.14	.69	none	none	none	1.00	.18	2.14	7.07	15.10	1851-52	17.93
1852	.58	.12	6.40	.19	.30	none	none	none	sprin	none	6.00	13.41	27.00	1852-53	36.36
1853	3.00	2.00	7.00	3.50	1.45	sprin	sprin	none	sprin	sprin	1.50	1.54	19.99	1853-54	20.06
1854	3.25	8.50	3.25	1.50	.21	.31	none	sprin	sprin	1.01	.65	1.15	19.83	1854-55	18.62
1855	2.67	3.46	4.20	4.32	1.15	.01	none	none	sprin	none	.75	2.00	18.56	1855-56	13.76
1856	4.92	.69	1.40	2.13	1.84	.03	none	none	sprin	.20	.65	2.40	14.26	1856-57	10.46
1857	1.38	4.80	.68	sprin	sprin	.35	none	sprin	none	.66	2.41	2.63	12.91	1857-58	15.00
1858	2.44	2.46	2.88	1.21	.20	.10	.01	sprin	sprin	3.01	.15	4.34	16.80	1858-59	16.03
1859	.96	3.91	1.64	.98	1.04	none	none	none	.02	none	6.48	1.83	16.86	1859-60	22.09
1860	2.31	.93	5.11	2.87	2.49	.02	.63	none	.06	.91	.18	4.28	19.19	1860-61	16.10
1861	2.67	2.92	3.32	.48	.59	.14	.55	none	none	sprin	2.17	8.64	21.38	1861-62	35.56
1862	15.04	4.26	2.80	.82	1.81	.01	none	.01	none	.36	sprin	2.33	27.44	1862-63	11.58
1863	1.73	2.75	2.36	1.69	.36	none	none	none	sprin	none	1.49	1.82	12.20	1863-64	7.87
1864	1.08	.19	1.30	1.08	.74	.09	none	.08	sprin	.12	6.72	7.87	19.27	1864-65	22.51
1865	4.78	.71	.48	1.37	.46	none	sprin	none	.08	.48	2.43	.36	11.15	1865-66	17.93
1866	7.70	2.01	2.02	.48	2.25	.10	.02	none	none	sprin	2.43	9.51	26.52	1866-67	25.30
1867	8.44	7.10	1.01	1.80	.01	none	none	none	.01	none	3.81	12.85	30.03	1867-68	32.79
1868	6.04	3.15	4.35	2.31	.27	sprin	none	none	none	none	.77	2.61	19.50	1868-69	16.64
1869	4.79	3.63	2.94	1.24	.65	.01	none	none	sprin	2.12	.85	1.96	18.19	1869-70	13.57
1870	1.37	3.24	1.64	2.12	.27	sprin	sprin	sprin	none	.02	.58	.97	10.21	1870-71	3.47
1871	2.08	1.92	.69	1.45	.76	sprin	none	none	sprin	.21	1.22	10.59	18.92	1871-72	23.65
1872	4.04	4.74	1.94	.61	.28	.02	none	none	sprin	.22	1.93	5.39	19.17	1872-73	14.21
1873	1.23	4.36	.55	.51	none	sprin	.02	sprin	none	.31	1.21	10.01	18.20	1873-74	22.90
1874	5.20	1.86	3.05	.89	.37	sprin	sprin	none	.05	2.26	3.80	.44	17.92	1874-75	17.70
1875	8.70	.55	.80	sprin	sprin	1.10	none	none	none	.44	6.20	5.52	23.31	1875-76	26.53
1876	4.99	3 75	4.15	1.10	.15	none	.21	.02	sprin	3.45	.30	none	18.12	1876-77	8.96
1877	2.77	1.04	.56	.19	.64	.01	sprin	sprin	none	.73	1.07	1.43	8.44	1877-78	24.86
1878	9.26	8.04	3.09	1.07	.17	none	none	none	.29	.55	.51	.47	23.45	1878-79	17.85
1879	3.18	3.88	4.88	2.66	1.30	.13	sprin	sprin	none	.88	2.05	3.41	22.37	1879-80	26.47
1880	1.64	1.83	1.70	14.20	.76	none	sprin	none	none	none	.05	11.81	31.99	1880-81	26.57
1881	6.14	5.06	1.37	1.64	sprin	.50	sprin	none	.30	.55	1.88	3.27	20.71	1881-82	16.51
1882	1.89	2.40	3.78	1.99	.35	.10	sprin	none	.57	2.63	3.22	1.13	18.06	1882-83	18.11
1883	2.23	1.11	3.70	.67	2.85	none	none	none	.90	.97	.61	.44	13.48	1883-84	24.73
1884	3.43	4.46	8.14	4.32	.06	1.45	none	sprin	.60	2.01	none	10.45	34.92	1884-85	16.58
1885	2.16	.49	.08	.68	sprin	.11	sprin	none	.08	.02	11.34	5.76	20.72	1885-86	32.27
1886	7.95	.29	2.68	4.08	.07	none	none	none	none	.68	.21	2.21	18.17	1886-87	13.97
1887	1.12	6.28	.94	2.53	sprin	none	none	sprin	.02	none	.45	2.09	13.43	....	....
Totals	143.31	109.74	108.76	74.07	25.79	4.59	1.14	.11	4.23	26.48	80.46	176.49	738.67	....	752.32
Av'g's	*3.771	*2.888	*2.809	*1.949	*.678	*.121	*.030	*.003	†.108	†.690	†2.063	†4.525	*19.43	....	*19.79

*Averages for 38 years. †Averages for 39 years.

Barometer Vergleichungen. Ausgefuhrt in den Jahren 1886–1887, von A. F. Sundell. Helsingfors, 1887.—In this volume of 64 quarto pages with plate of illustrations, we have an account of the most extensive series of comparisons of Normal Barometers that has ever been undertaken by any individual. The barometers at seventeen observatories having been compared with the travelling barometer, Professor Sundell has come the nearest of any one to carrying out Professor Wild's idea of comparing the standard barometers of all the principal meteorological observatories of Europe. That Professor Sundell accomplished this work without any accident to his barometer (except the spilling of a little mercury at Berlin) shows the great care taken by him in order to secure accurate results. Any one who has not tried it, little knows the difficulty in traveling with a barometer through countries where foreign languages are spoken, and where custom-house officials insist on seeing all there is to be seen of a traveler's baggage.

The traveling barometer of Professor Sundell was of peculiar construction. He took a reserve glass tube of a Wild-Fuess control barometer, and by attaching a series of glass tubes to the two ends of this tube, he made a barometer which can be emptied and refilled with very little trouble. (He had constructed a barometer on the same principle in 1885. Described in the Acta Societatis scient. Fenn., T. XV, p. 387.)

The air is expelled from the top of the barometer tube by causing the mercury to pass through the tube into a second one connected with it by a capillary tube. Sundell gives the details of construction in Figures 1–5; Fig. 7 being a plan of the Helsingfors normal barometer by the same physicist, and having a method of reading somewhat similar to that proposed by Thiesen.

The traveling barometer was emptied of the mercury during journeys, and was refilled at each place of observation; and the amount of gas pressure in the vacuum chamber determined each time by the application of Mariotte's law to the varying volumes of the vacuum space.

For gas tension of from $0.07^{mm.}$ to $0.37^{mm.}$ in the vacuum

chamber, Sundell found in a series of experiments that the results from his barometer did not differ from the standard by more than would be expected of a first-class barometer of ordinary construction.

The smallness of the average deviation of the single readings in the various comparisons shows the great care taken. The amount seldom exceeded ± 0.025$^{mm.}$ and many times it was less than this.

Sundell, 1886-1887.		1886-7. Sundell.	1882-3. Signal Service.	1881. Chistoni.	Sig. Serv. —Chistoni.	Sig. Serv. —Sundell.	Chistoni. —Sundell.
C. O. N.—Stockholm (Pistor, Martins 579)	=	mm +.06		+.00			+.06
C. O. N.—Christiania (Wild, Fuess 214)	=	+.07					
C. O. N.—Copenhagen (Wild, Fuess 87)	=	+.13		—.11*			+.24*
C. O. N.—Hamburg (Koppen, Fuess 9)	=	—.31	—.50	—.35	+.15	+.19	+.04
C. O. N.— " (Wild, Fuess 79)	=	+.05					
C. O. N.—Brussels (Tonnelot)	=	+.22					
C. O. N.—Utrecht (Becker)	=	—.44					
C. O. N.—Kew (Newman 34 + .002 in. = st'd)	=	—.05	—.10	—.00**	+.10	+.05	—.05
C. O. N.—Paris Met. Bur. (Alvergniat)	=	+.12	—.09	—.08	+.01	+.21	+.20
C. O. N.— " (Tonnelot 1572)	=	+.54					
" (Regnaults nor.)	=	+.16	—.05	—.04	+.01	+.21	
C. O. N.—Sevres Int. Bur. (Turrettini)	=	+ 25	—.10			+.35	
C. O. N.— " " (Wild, Marek)	=	+.21	—.20			+.41	
C. O. N.— " " (Chappius)	=	+.32	(—.22)			+.54	
C. O. N.—Zurich (Wild, Fuess 168)	=	+.09					
C. O. N.— " (Fuess, Syphon)	=	—.03					
C. O. N.—Munich (Wild, Fuess 43)	=	+.08					
C. O. N.— " (Wild, Fuess 4)	=	+.00					
C. O. N.—Vienna (Pistor 279)	=	+.13	—.08	—.17	—.09	+.21	+.30
C. O. N.—Chemnitz (Wild, Fuess 163)	=	—.01					
C. O. N.—Berlin Met. Inst. (Wild, Fuess 79)	=	+0.08					
C. O. N.—Berlin Met. Inst. (Wild, Fuess 76)	=	—.03	—.04			+.01	
C. O. N.—Berlin N. A. K. (Wild, Fuess 79)	=	+.01					
C. O. N.—Berlin N. A. K. (Wild, Fuess 229)	=	—.05	***				
C. O. N.—Hamburg (Köppen, Fuess 9)	=	—.33					
C. O. N.— " (Köppen, Fuess 10)	=	+.03					
C. O. N.—Helsingfors (Wild, Fuess 99)	=	—.27					
C. O. N.—St. Petersburg	=	—.00					
C. O. N.—Helsingfors (Wild, Fuess 129)	=	+.03					
C. O. N.—Helsingfors (Wild, Fuess 129)	=	+.18)					
C. O. N.— " (Cassella 1155)	=	—.13					
C. O. N.— " (Girgensohn)	=	—.41					

*Chistoni compared Barometer "Jünger" at Copenhagen, and the reviewer is uncertain if this barometer agreed with Wild, Fuess No. 87.

**I have not access to Chistoni's original paper, but I am informed by a friend that the values for Paris and Kew were not obtained by direct comparisons with Chistoni's barometer, but through other barometers which had been compared at these places.

***The Signal Service comparisons gave C. O. N.—Berlin N. A. K. Fuess Nor. = —.25.

The above table shows Sundell's results as referred to Wild's normal barometer at the Central Physical Observatory, St.

Petersburg, which is designated by the letters C. O. N. In order to see how his results compare with some other recent comparison of barometers, there are added the Signal Service* observations of 1882–3, and those made by Chistoni in 1881 and 1882 (?). (See Annali della Met." Part I, 1881).

Column 1 gives Sundell's values, column 2 gives the Signal Service results, and column 3 shows Chistoni's values. Column 4 shows the difference between the Signal Services and Chistoni; column 5, that between the Signal Service and Sundell; and column 6 that between Chistoni and Sundell.

In the last three columns the sign + signifies that the first named comparison (of each pair) gave the higher reading to the barometer at the place of comparison.

It will be seen that these more modern inter-comparisons of standard barometers do not agree any better than the older comparisons. F. WALDO.

HEIGHTS OF CLOUDS.—The cloud illumination caused by the electric lights of Detroit and Ypsilanti are occasionally so well defined in outline, as seen from this Observatory, that it occurred to the Director to inaugurate a series of altitude measurements for the purpose of determining the heights of all forms of clouds visible at Ann Arbor after twilight.

The central portion of Detroit is about 35 miles from the Observatory, while Ypsilanti is only 5.8 miles distant. The azimuths of the two cities differ by about 35°, so that the conditions for determining the heights of the upper and lower clouds can always be made favorable when the atmosphere is sufficiently transparent. When the clouds are very high, the Detroit illumination is so well defined that the probable error of a single measurement of an altitude is only a few minutes of arc. When the clouds are low, the nearer illumination is well defined and the farther one either invisible, or coincident with the apparent horizon. A detailed statement of the method of observation, together with the separate results obtained, will be given in a later number of the JOURNAL. The greatest and least heights recorded up to the present time are respectively 17,580 and 770 feet.

* See report by the writer in the Met. Zeitschrift for 1887, p. 98; and Monthly Weather Review, April, 1887.

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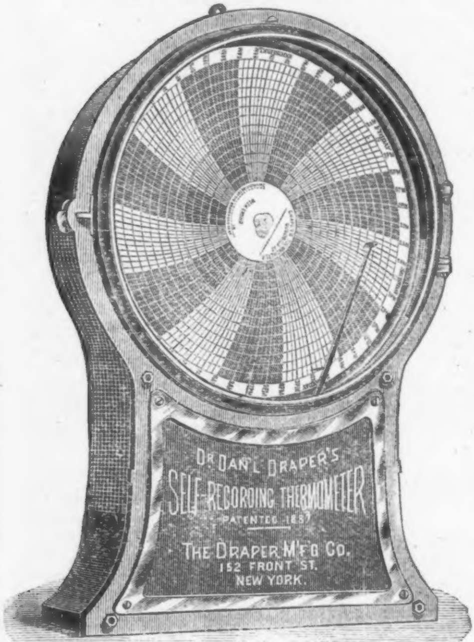
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